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ABSTRACT

S. Jackson and S. Jacobs's criticism of "single message" designs in communication research served as a framework for a study that examined the differences between various sequential request paradigms. The study sought to answer the following questions: (1) What were the most naturalistic request sequences assured to replicate "foot-in-the-door" (progressively reasonable requests) and "door-in-the-face" (an outrageous request followed by more reasonable requests) effects? (2) To what extent did the subjects' perceptions of request-effort and request-novelty collide and result in different compliance probabilities? and (3) How stable were subjects' impressions of the request sequences and to what degree were the subjects in agreement concerning request characteristics? Subjects, 84 intermediate-level speech communication students, were presented with a range of requests involving a number of different target behaviors. Results showed that subjects exhibited different compliance rates and perceptions about the novelty and/or effort associated with requests. However, the extent to which subjects' responses did not follow a pattern suggestive of a systematic convergence or divergence of perceptions associated with the requests could not be entirely accounted for by either dimension alone, or by a simple adaption level explanation. The study achieved a better understanding of which request sequences would be most natural in college situations. (Tables of data and a four-page reference list are included.) (JD)

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SEQUENTIAL REQUESTS AND THE PROBLEM OF MESSAGE SAMPLING

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Sequential Requests and the Problem of Message Sampling

Abstract

Jackson and Jacobs' (1983) indictment of the use of "single message" designs in communication research is employed as a framework for analysing previous research in the sequential request paradigm. After questioning the validity of previous studies of foot-in-the-door and door-in-the-face techniques in light of the "language-as-fixed-effects" fallacy position, a study is reported which examines differences between various sequential request exemplars. Subjects are found to exhibit different compliance rates and perceptions of the novelty and/or effort associated with requests. The results are discussed in terms of their implications for future sequential request investigations.

Sequential Requests and the Problem of Message Sampling

Recently, Jackson and Jacobs (1983) have argued that our field's propensity to rely upon "single message" designs in the conduct of research makes suspect a host of previous generalizations about human communication. Reasoning that the wide variability among messages drawn from a language community precludes our ability to treat specific cases as representative of a message category (insofar as they are treated as "fixed-effects"), Jackson and Jacobs suggest that "suspicions should be doubly aroused when a line of research produces contradictory findings from one study to the next" (p. 170). Unfortunately, even a brief inspection of our literature in domains as diverse as interpersonal influence (Seibold, Cantrill, & Meyers, 1985), the use of evidence in argumentation (Kellerman, 1980), and communication accuracy (Mehrabian & Reed, 1968) reveals numerous instances of such conflicting results. As suggested, much of this empirical muddle may be the result of our overzealous attempts to explain the complexity of communication by recourse to simple designs, straightforward answers, and an abiding faith in the ability to intuit which messages best represent a language category.

In their summation, Jackson and Jacobs contend that communication scholars should reexamine the bases for "many well-entrenched (but thinly supported) claims about communication" (p. 178). In the following pages, we offer a preliminary example of how such an inquiry may be profitably conducted. Our goal is to demonstrate both the pride and prejudice of an established line of interpersonal influence research by (1) briefly reviewing the implications of the "language-as-fixed-effects fallacy" (Clark, 1973) for communication research, (2) examining the empirical findings and indictments associated

with the sequential request paradigm, and (3) detail the results of a study designed to investigate the variability between instantiations of foot-in-the-door and door-in-the-face techniques of interpersonal influence.

Communication Research and the Problem of Message Sampling

Among other issues, communication scholars are often concerned with the effect that particular types of messages have upon human conduct. Whether concentrating on attitude change phenomena, patterns of interaction, or impression formation the primary manipulation in such studies generally involves the presentation of specific appeals, arguments, or messages to a target audience. Typically, messages are selected and/or constructed in light of their representativeness of some abstract category of strategems. For example, investigations of the use of fear appeals to induce attitude change often employ arguments designed to evoke an anxiety reaction in a group of target individuals. However, in most cases, researchers are not interested in the effect of particular messages per se; rather, it is hoped that a generalization can arise out of the empirical results.

Unfortunately, the use of single messages to represent abstract categories is fraught with empirical dangers. Most serious is the possibility that variations in the data derrived from a message analytic study or experiment are largely due to the particular messages employed rather than the properties of the abstract category they are meant to represent. Even to the extent that multiple investigations of the abstract category occur, it is unlikely that such replications will allow researchers to generalize findings unless a random-effects model is specified in the statistical analysis of the data. That is, the researcher must either randomly sample the abstract category for representative examples or reduce the probability of Type I error by system-

atically accounting for the error variance linked to differences between messages. Clark (1976) observes that to do otherwise, to fallaciously analyse the data as if the effects were "fixed" for all cases of the abstract category, is to invite the worst of inferential errors since the probability of chance findings is grossly exacerbated (cf., Jackson & Jacobs, 1983; Santa, Miller, & Shaw, 1979). Alternatively, detractors of the language-as-fixed-effects fallacy position (e.g., Bradac, 1983; Hewes, 1983; Wike & Church, 1976) argue that the random selection of messages is a practical impossibility. Not only do theoretical dictates often force the researcher to choose some instantiations while eschewing others but, even if the selection of messages is not theory-driven, we lack the wherewithal to truly sample a language population in a random fashion. Hence, the dilemma some communication researchers face is relatively simple: Either the research task is hopelessly atomized into non-generalizable findings or it is bogged down in the mire of searching for an elusive random sample of message exemplars.

Although the problem of message sampling should be a serious concern for those in our field, its dangers are not intractable. Jackson and Jacobs (1983) suggest (1) avoiding single-case designs in favor of procedures which simultaneously assess the communication impact of multiple messages drawn from the same category, (2) treating differences between the effects of messages drawn form a single language population as error variance rather than a systematic effect of the abstract characteristic being studied, and (3) the message samples be evaluated in light of their prototypicality, inter-case diversity, and naturalness. Arguably, through procedures such as these and others (e.g., use of quasi-f tests; random-effects statistical analyses), we can improve the generalizability of those studies which take message variables

as central to their domain of inquiry.

As may be apparent, however, some of the procedures noted above rest upon somewhat shaky assumptions or could be cumbersome. First, given the all too limited resources (e.g., subjects) available to social scientists, the balance between employing multiple instantiations of a message strategy in a single study and the desire to detect statistical differences among experimental conditions (i.e., statistical power) often tips in favor of the later concern. Second, some researchers may find that the more interesting results are associated with detecting differences between instantiations of strategies rather than with the strategies themselves. In this case, treating such differences as error variance in a random effects model obscures the meaningful assumption that some message samples are more prototypical than others. And third, we may be mistaken to assume that even prototypicality can be determined a priori and apart from the context of unique perceptions subjects use to interpret the the messages they are given. In fact, we know comparatively little about the phenomenal world of our subjects; in the absence of such information it becomes difficult to trust our beliefs concerning what they consider to be representative, diverse, and natural message examples (cf. Folger & Poole, 1981).

Of course, none of the objections we raise here deny the validity of Jackson and Jacobs' (1983) challenge. Rather, we wish to underscore the difficulty of determining the representativeness of message samples prior to testing their effects and attempting to further the process of empirical generalization. A more practical alternative might be to test the applicability of a range of message cases for a given sample of subjects, determine which cases are most representative of the abstract language category by examining the subjects' underlying perceptions of the messages, and then use the most repre-

sentative cases in an experiment designed to explore particular research hypotheses. This procedure offers a number of benefits for researchers who confront resource limitations. On the one hand, it provides the researcher with tangible evidence for the validity of his/her manipulation of a language variable. To the same extent that representational validity tests (Folger & Poole, 1981) assess the correlation between imposed and naive codings of naturalistic conversation, a preliminary estimation of one's attempt to validly sample a language population can prove heuristic. On the other hand, this procedure approximates the accepted method of pre-testing experimental protocols in an attempt to insure that manipulations conform to the message-structure or content specifications embedded in underlying theoretical postulates. Finally, and as is the case with the present research, this method of message sampling may serve the dual purpose of establishing the representativeness of language exemplars as well as provide information useful in clarifying previously enigmatic issues.

Sampling Sequential Message Strategy Instantiations

As suggested earlier, some research programs associated with the study of interpersonal influence have resulted in conflicting reports concerning the effectiveness of various compliance gaining strategies. Furthermore, in some lines of research, these empirical contradictions may be partially or wholly due to the failure to appropriately sample instantiations representative of the strategy. According to Seibold et al. (1985), the problem of message sampling may undergird many of the empirical conflicts in the sequential request paradigm. In particular, the effectiveness of the foot-in-the-door (FITD) and door-in-the-face (DITF) techniques may depend upon how representative the message exemplars in a study are and the extent to which the abstract strategy

grounds the construction of request instantiations.

The FITD and DITF techniques are essentially mirror-images of each other, and their effectiveness is always gauged against non-sequential, similar critical requests. In the typical FITD scenario (e.g., Freedman & Fraser, 1966), targets are first asked to comply with a trivial initial request and then are asked for a more substantial degree of compliance. Alternatively, in DITF situations (e.g., Cialdini, Vincent, Lewis, Catalan, Wheeler, and Darby, 1975), individuals are supposed to reject an outlandish intitial request which is then followed by a more moderate critical request. When compared against compliance rates of control subjects who have only received the second request, we generally observe that experimental subjects given either FITD or DITF versions exhibit significantly higher compliance with the critical requests.

In the past decade, numerous research groups have examined the FITD and DITF techniques. Previous studies have considered the optimal magnitude of initial and critical requests (e.g., Even-Chen, Yinon, & Bizman, 1978), the length of delay between request presentations (e.g., Cann, Sherman, & Elkes, 1975), and the effect of varying the intent or topic of the requests (e.g., DeJong, 1981). A number of reviewers (Cantrill, 1985; DeJong, 1979; Seibold et al., 1985) have concluded that the FITD and DITF techniques are applicable to a wide variety of compliance-gaining situations. And recent meta-analyses (Beaman, Cole, Preston, Klentz, & Steblay, 1983; Dillard, Hunter, & Burgoon, 1984) suggest that both effects are fairly reliable across studies even though their utility is limited by only small increases in compliance rates as well as the need to insure that specific eliciting conditions are established.

Social psychologists and communication researchers have enlisted a number of theories and frameworks to champion a specific reason why the FITD

and DITF are effective. At one time or the other, self-perception (e.g., Freedman & Fraser, 1966), situation-perception (e.g., Rittle, 1981), social labeling (e.g., DeJong, 1981), perceptual contrast (e.g., Cantrill, 1985), reciprocal concession (e.g., Cialdini et al., 1975) or self-presentation (e.g., Pendelton & Bateson, 1979) processes have been used to explain and/or predict sequential request effectiveness. Nonetheless, our attempts to understand the generative mechanism of FITD and DITF compliance have not been wholly successful. First, some researchers (e.g., Tybout, 1978) have failed to establish the effectiveness of both techniques in a single study. Second, designs have sometimes been employed which lack the sophistication to adequately test the predictions of a given theory (e.g., Miller, Seligman, Clark, & Bush, 1976; cf., Cantrill, 1985). Third, additional research has not yielded corroborative findings regarding the explanatory power of a conceptual framework (e.g., Scott, 1977). Fourth, some research groups have confounded differences between the effort (e.g., time volunteered) associated with initial and critical requests with differences in the perceived novelty of the separate requests (e.g., Shanab & O'Neil, 1982). And fifth, reviewers generally agree that no single perspective can simultaneously account for the effectiveness of both techniques (cf., Dillard & Burgoon, 1982). Hence, even though we may be satisfied that the FITD and DITF techniques can be effective, we should suspect their utility for interpersonal influence campaigns until such time as we can specify why the effects occur.

One of the problems historically associated with research in the sequential request paradigm has been the seeming lack of rigor in the construction of messages used to instantiate the abstract strategies. Rarely have researchers indicated their reasons for assuming that a particular request sequence was an appropriate example of the FITD or DITF scenario. Manipulation checks have not

been made; topics and dimensions of perception (i.e., effort- versus novelty-differences) have been crossed; few attempts have been undertaken to replicate or diversify the examination of naturally occurring request sequences. In fact, many researchers seem to have determined crucial questions of message appropriateness post hoc: The requests are deemed representative precisely because they resulted in higher compliance rates and not because they are valid examples of the abstract strategies they are meant to represent. Viewed from this perspective, a major indictment of previous sequential request research can be couched in the language-as-fixed-effects analysis presented earlier.

In light of the many problems associated with sequential request research, a study was conducted to assess the appropriateness of a number of FITD and DITF exemplars. By presenting subjects with a range of requests involving a number of different target behaviors we hoped to resolve three fundamental issues. First, what were the most naturalistic request sequences assured to replicate FITD and DITF effects? Second, to what extent did the subjects' perceptions of request-effort and request-novelty collide and result in different compliance probabilities? And third, how stable were subjects' impressions of the request sequences and to what degree were the subjects in agreement concerning request characteristics?

Testing FITD and DITF Message Exemplars

Subjects. The subjects for this study were contacted in the winter of 1984 and were members of four intermediate level (i.e., predominantly juniors and seniors) speech communication classes at a large midwestern university. In all, a total of 102 students representing various majors participated in the first wave of data collection; 89 of these provided responses during the second contact session. Only the responses gathered from subjects who were present for

both the test and the retest were used in the data analysis. Furthermore, five cases were eliminated from the analysis due to their inability to follow directions. Thus, the sample consisted of 22 males, 58 females, and four subjects of unknown gender, all of whom participated on a volunteer basis.

Procedures. In order to provide subjects with a range of requests which varied along the effort-novelty axes, we first constructed a number of message exemplars which provided for a diversity of levels. Initially, we selected four topic areas we believed represented a range of issues our subjects might be familiar with. We assumed that college students regularly receive requests for health donations such as assisting in blood drives. We also expected a degree of familiarity with requests to assist the elderly in a variety of ways. And, though not as commonplace, we anticipated that requests for marketing tests such as for clothing and food would appear natural in a university setting. Next, we operationalized the degree of request-effort as equivalent to the amount of time required by compliance. Recognizing that Even-Chen et al. (1978) established that an initial request in the DITF scenario should not be so great as to induce reactance, and assuming that Seligman, Bush, and Kirsch (1976) were correct in asserting that there exists a basement-level of initial compliance necessary to induce the FITD effect, we asked subjects to volunteer either fifteen minutes, a single hour, or one hour per day for three weeks. Finally, we constructed requests that would cover a range of severity regarding the topics' novelty: soliciting health donations via the telephone (from blood to sperm and ova donations); helping the elderly (from talking with to assisting bathe); trying different types of clothes (from sweaters to underwear); eating odd foods (from processed meats to uncooked seaweed). A schematic representation of the various requests used in this study is found in Table I.

Once we had devised a series of requests, written protocols were constructed which allowed for the easy substitution of different request characteristics. Topic areas and various combinations of the effort-novelty levels were embedded in the following generic exemplar:

> Assume that an individual you do not know visits your class one day and says: Hello, my name is (a fictitious name) and I am conducting some research for Dr. (a fictitious name) of the (request congruent agency). We are interested in obtaining a number of students to volunteer (X amount of time) to (do something or other). Would you like to volunteer for this research?

Each request scenario was followed by seven scales that examined (1) the base-rate compliance level associated with the exemplar, (2) its perceived level of effort, and (3) its perceived novelty. Each scale consisted of a six-point, bi-polar adjective range of interval-level values measuring the following items:

1. How likely is it you would comply with this request?
2. How much effort would it take to comply with this request?
3. How unusual is this request?
4. How hard would it be for you to comply with this request?
5. How unique do you think a request of this sort is?
6. How much of a problem would it be to comply with this request?
7. How novel is this request?

Booklets were assembled which contained eighteen request instantiations, completely crossed by all levels of requests, representing various degrees of effort and novelty for two of the topic areas. The order of the scales and requests were randomized across booklets and between contact groups.

With the various conditions of the study represented by a variety of booklets, intact classes of students were randomly assigned to one of two groups: students in two classes (n = 39) were presented with request scenarios associated with the clothing and food topics while the other two classes (n = 45) received the health and elderly issues. For all classes, a male experimenter was introduced by the instructor of the course, booklets were distributed, and the subjects were instructed to indicate their impressions of each request scenario on the scales provided in the protocols. The subjects completed the task in 15-30 minutes and were again contacted for the retest 7-14 days later. After this second session, the subjects were debriefed and requested not to discuss the research.

Data analysis. The first issue our analysis of the data was meant to resolve concerned which of the topic areas and requests sequences best represented FITD and DITF scenarios. To select the most prototypic FITD and DITF exemplars, the "liklihood of compliance" means for each request were examined in light of (1) which combination of like-topic requests exhibited both the highest and lowest compliance ratings and (2) reflected the pattern of responses most similar to typical sequential effects. To be considered an adequate representative of a FITD sequence, a request had to receive a high degree of compliance under conditions of low-novelty and moderate-effort as well as under conditions of low-effort and moderate-novelty. Alternatively, representative DITF requests had to receive a low degree of compliance under conditions of high-novelty and moderate-effort as well as when the request was of high-effort and moderate-novelty. Finally, both the FITD and the DITF request stimuli had to receive a relatively low degree of compliance under moderately-novel and moderately-effortful (i.e., typical control) conditions. These procedures offered the

best prospectus for identifying topics that both kept the issue of compliance consistent across request stimuli and could result in FITD and DITF effects.

Examination of Table II reveals that the four topic areas exhibited different levels of compliance-likelihood means. Clearly, the topic dealing with requests concerning the sampling of different foods was not representative of typical sequential effects. Not only did it result in the lowest compliance means in the FITD instantiation ($\bar{x}$ = 2.19) but there also appears to be an inverted curvilinear trend in the compliance levels associated with the different degrees of novelty. Similarly, the mediocre likelihood of compliance ratings accompanying the clothing topic ($\bar{x}$ = 2.96), though demonstrating the predicted linear trend (i.e., as effort or novelty increases, compliance declines), did not seem suitable evidence of its worth as an exemplary FITD or DITF topic area. On the other hand, the health and elderly requests adequately met the criteria noted earlier, even though an inspection of the data indicated that these sequences were not perfect exemplars. First, the health and elderly topics received the highest compliance ratings of all the request scenarios for the FITD technique ($\bar{x}$ = 3.76 and $\bar{x}$ = 4.40 respectively). Also, even though the DITF stimuli for these topics did not result in compliance ratings lower than those associated with the food and clothing topics ($\bar{x}$ = 2.16 and 2.72 versus $\bar{x}$ = 1.68 and 1.78), the compliance levels seem low enough to qualify the requests as acceptable DITF exemplars. And, despite the fact that the moderately severe scenarios for neither the health nor the elderly topics resulted in exceptionally low compliance levels ($\bar{x}$ = 3.52 and $\bar{x}$ = 4.36), the overall pattern of responses seem superior to those associated with the other request sequences. Hence, in this study, our subjects' ratings of the health and elderly requests established that these topic areas were more prototypic of sequential request scenarios than

were the food and clothing requests.

The second question undergirding this research considered the extent to which the effort- and novelty-dimensions were unique vis a vis the subjects' perceptions of the various request exemplars. To assess the orthogonality of the two dimensions the effort and novelty scales for all of the request stimuli in each topic were correlated to determine if the scales were associated with a single underlying dimension of request perception. A strong Pearson-r correlation between the two sets of scales would constitute prima facie evidence that the effort and novelty dimensions were inherently confounded in the data.

Examination of Table III reveals the degree to which the effort and novelty scales were correlated. Even though the Pearson-r correlations for the health (.02 to .71) and elderly (.23 to .75) request scenarios' scales are generally higher than those associated with the other issues (.17 to .64 and .05 to .50), there are a number of factors which suggest that the two sets of scales are meaningfully confounded to varying degrees throughout the range of request scenarios. First, there does not appear to be a pattern in the occurance of significant correlations across the data set; we do not observe, for example, a collapsing of perceptual dimensions as the severity of requests increases. Second, the size of the correlations do not seem to depend upon the severity of the instantiations and, as is the case with the "easiest" request scenario in the clothing topic, may bear only an obscure relationship to the positivity of the association between the effort and novelty scales. Third, although the significant coefficients might be an artifact of subjects observing both sets of scales on the same pages of the booklets, these moderate to strong correlations occur at all levels of requests. Overall, then, the association between the two dimensions suggests that subjects' perceptions of different requests

may have been non-systematically conditioned by their impressions of either the effort and/or novelty manipulations contained in separate request protocols.

The third research question involved an assessment of how stable were the subjects' perceptions of various requests. Two analyses proved heuristic. First, a split-half reliability estimation test (i.e., Cronbach's-alpha) was employed to demonstrate the degree to which subjects were uniformly perceiving differences between request instantiations during the first wave of data collection. Second, a comparison of initial and retest responses would establish the stability of their perceptions over time. Hence, this last phase of analysis probed the perception of requests both between and within subjects.

The results of the split-half and temporal stability analyses are displayed in Table IV. Whereas the range of Cronbach's-alpha coefficients for the food (.77 to .95), the health (.84 to .97), and the elderly topics (.80 to .91) seem fairly strong, those associated with the clothing topic (.04 to .98) show greater variability and a rather dismal coefficient for the only negatively correlated request scenario in the set. However, the test-retest analysis demonstrated that these reliabilities were not exceptionally consistent over time. Subjects' stability measures ranged from .31 to .83 for the health requests, from .25 to .84 for the elderly scenarios, from .26 to .72 for the food requests, and from .45 to .82 for the clothing topic. Furthermore, there is no obvious linear trend to which level of requests results in higher stability measurements; in some cases less severe requests result in more consistent responses while in others it is the more severe situations that seem more stable. Thus, this analysis shows that although subjects were fairly consistent in their perceptions of the requests regarding their inter-subjective ratings, they were only moderately consistent in their perceptions over time.

Discussion and Implications for Message Exemplar Selection

In this study, we attempted to probe some of the underlying dimensions by which subjects perceive requests so as to better select diverse, natural, and representative message exemplars. To this end, we examined subjects' propensity to comply with a variety of requests in four topic areas, the manner in which they perceived the unique characteristics of novelty and effort variations embedded in the requests, and the extent to which the impressions were consistent across subjects and stable over time. Analysis of the data revealed that two of the topic areas were associated with request sequences (i.e., the health and elderly topics) that were more likely to produce prototypic FITD and DITF effects. Furthermore, subjects' perceptions of request novelty and effort were shown to possess nonorthogonal properties in only some of the requests; the association between measurements of effort and of novelty varied in a nonsystematic fashion. Finally, although subjects' impressions of request novelty and effort were reasonably consistent within experimental groupings, there was a much greater dispersal of ratings when we returned to reassess their perceptions. As with before, the stability measure did not exhibit a trend which would suggest obvious explanations for these results.

In summation, this research resulted in useful information. First, we have a better understanding of which request sequences would prove to be natural in a college situation. While the results of the study by and large corroborate our initial assumptions regarding how familiar subjects would be with the topic of the requests, an inspection of particular request perceptions showed some of the dangers of intuition. For example, an examination of Table II further reveals that, at least for the clothing topic, the predicted trend in liklihood of compliance ratings did not occur. Although we assumed that students would

find the consumption of uncooked seaweed more novel qua repulsive than the eating of raw fish such as sashimi, the responses betrayed their contrary expectations. Second, it is apparent that the independent manipulation of effortful or novel request characteristics proved elusive in this study. To the extent that subjects' responses did not follow a pattern suggestive of a systematic convergence or divergence of perceptions rooted in the severity of the requests, we cannot assume that either dimension was uniquely responsible for differences in the compliance ratings. Third, even though we might be tempted to conjure-up an adaptation level explanation (cf., Helson, 1964) for the lack of stability between first- and second-wave responses (i.e., the presentation of the first set of protocols anchored the perception of the second set), given the randomness of such changes in consistency we cannot assume that particular requests were inherently novel or effortful at some observable level of measurement. The important issue to bear in mind, however, is that we would not be aware of these differences between requests, or the discrepancies among subjects' perceptions, had we not challenged the commonsense assumptions embedded in many sequential request studies.

The results of this research have a number of implications for the study of sequential requests. We have offered a practical method for selecting appropriate requests to be employed in the further investigation of sequential request phenomena. Aside from testing the veracity of a researcher's intuitions, the method paves the way for specific empirical extensions. For example, the two topics which received the most prototypical liklihood of compliance ratings in this study were subsequently employed in research designed to examine the perceptual contrast explanation for sequential requests (Cantrill, 1985). In that experiment, subjects' compliance rates with FITD and DITD requests were

more than double those associated with control conditions, anchoring-contrast effects were not observed in subjects' ratings of the requests, and multivariate analyses of variance indicated that request perceptions were generally conditioned by a combination of topic, request sequence, and effort versus novelty manipulation factors. Hence, the procedures outlined earlier may have directly contributed to the utility of a later study.

At a more fundamental level, the results of this research offer a possible explanation for various discrepencies in the sequential request paradigm. For example, Shanab and Isonio (1980) attempted to demonstrate the effectiveness of the DITF technique using "socially undesirable" requests and delaying the presentation of the critical request for an average of 8.5 days. They did not observe a sequential request under these conditions. However, the researchers may have confounded differences between the effort associated with initial and critical requests (i.e., volunteer two hours per day for three months versus volunteer only two hours) and the novelty of the requests (i.e., electroshock humans versus electroshock rats). Though the finding of no DITF effects under delay conditions is consistent with other research findings (e.g., Cialdini et al., 1975), most studies of delay effects have exhibited similar problems in the crossing of perceptual dimensions. And, since the timing of the critical request has previously been assumed to only affect DITF compliance and not FITD responses, future investigations may wish to replicate these studies without confounding differences between effort and novelty dimensions.

In conclusion, and as Jackson and Jacobs (1983) suggest, we would urge our colleagues to seriously reexamine the empirical bases for much of our knowledge about human communication. Certainly, research traditions other than those associated with the study of interpersonal influence may also be analysed

in light of the language-as-fixed-effects fallacy position. We may find that, with due caution, we can use previous research endeavors as springboards for even more fruitful empirical research that takes into account the complexity of our subject matter. It is doubtful that a robust conception of human communication will be arrived at easily or without recourse to diverse, natural, and representative slices of the language community to work with.

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Table I

Request Scenarios

	FOOD	CLOTHING	HEALTH	ELDERLY
low effort/ low novelty	15 min. of tasting processed meats	15 min. of trying on sweaters	15 min. calling for blood	15 min. talking with elderly
low effort/ med. novelty	15 min. of tasting raw fish	15 min. of trying on sleepwear	15 min. calling for organs	15 min. running elderlys' errands
low effort/ high novelty	15 min. of tasting uncooked seaweed	15 min. of trying on underwear	15 min. calling for sperm/ova	15 min. instruct how to bathe
med. effort/ low novelty	1 hour of tasting processed meats	1 hour of trying on sweaters	1 hour calling for blood	1 hour talking with elderly
med. effort/ med. novelty	1 hour of tasting raw fish	1 hour of trying on sleepwear	1 hour calling for organs	1 hour running elderlys' errands
med. effort/ high novelty	1 hour of tasting uncooked seaweed	1 hour of trying on underwear	1 hour calling for sperm/ova	1 hour instruct how to bathe
high effort/ low novelty	2 hrs. for 3 wks. tasting meats	2 hrs. for 3 wks. trying sweaters	2 hrs. for 3 wks. for blood	2 hrs. for 3 wks. talking with
high effort/ med. novelty	2 hrs. for 3 wks. tasting raw fish	2 hrs. for 3 wks. trying sleepwear	2 hrs. for 3 wks. for organs	2 hrs. for 3 wks. running errands
high effort/ high novelty	2 hrs. for 3 wks. tasting seaweed	2 hrs. for 3 wks. trying underwear	2 hrs. for 3 wks. for sperm/ova	2 hrs. for 3 wks. instructing bathe

Table II

Likelihood of Compliance Means

Topic	Situation	Compliance Likelihood
Food	Low-effort & Low-novelty	3.48
	Low-effort & Medium-novelty	2.19
	Low-effort & High-novelty	2.22
	Medium-effort & Low-novelty	2.93
	Medium-effort & Medium-novelty	1.78
	Medium-effort & High-novelty	2.30
	High-effort & Low-novelty	1.96
	High-effort & Medium-novelty	1.68
	High-effort & High-novelty	1.52
Clothing	Low-effort & Low-novelty	3.85
	Low-effort & Medium-novelty	2.96
	Low-effort & High-novelty	2.19
	Medium-effort & Low-novelty	3.26
	Medium-effort & Medium-novelty	2.74
	Medium-effort & High-novelty	1.82
	High-effort & Low-novelty	2.04
	High-effort & Medium-novelty	1.78
	High-effort & High-novelty	1.78
Health	Low-effort & Low-novelty	4.20
	Low-effort & Medium-novelty	3.76
	Low-effort &	2.12

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Topic	Situation	Compliance Likelihood
Health	Medium-effort & Low-novelty	4.04
	Medium-effort & Medium-novelty	3.52
	Medium-effort & High-novelty	2.28
	High-effort & Low-novelty	2.24
	High-effort & Medium-novelty	2.16
	High-effort & High-novelty	1.60
Elderly	Low-effort & Low-novelty	4.80
	Low-effort & Medium-novelty	4.40
	Low-effort & High-novelty	2.24
	Medium-effort & Low-novelty	4.12
	Medium-effort & Medium-novelty	4.36
	Medium-effort & High-novelty	2.04
	High-effort & Low-novelty	2.92
	High-effort & Medium-novelty	2.72
	High-effort & High-novelty	1.72

Table III

Correlations Between Effort

and Novelty Scales

Situation	Topic			
	Food	Clothing	Health	Elderly
Low-effort & Low-novelty	.38*	-.38*	.44*	.23
Low-effort & Medium-novelty	.38*	.35*	.67*	.28
Low-effort & High-novelty	.17	.50*	.18	.64*
Medium-effort & Low-novelty	.50*	.05	.54*	.30
Medium-effort & Medium-novelty	.46*	.46*	.37*	.57*
Medium-effort & High-novelty	.40*	.50*	.45*	.75*
High-effort & Low-novelty	.24	.47*	.70	.45*
High-effort & Medium-novelty	.42*	.38*	.71	.35*
High-effort & High-novelty	.64*	.34*	.02	.66*

* $p < .05$

Table IV

Reliability and Stability Measures

Topic	Situation	Interitem Reliability	Temporal Stability
Food	Low-effort & Low-novelty	novelty = .93 effort = .93	.66 .68
	Low-effort & Medium-novelty	novelty = .93 effort = .95	.72 .59
	Low-effort & High-novelty	novelty = .92 effort = .83	.26 .26
	Medium-effort & Low-novelty	novelty = .94 effort = .82	.47 .47
	Medium-effort & Medium-novelty	novelty = .92 effort = .87	.67 .67
	Medium-effort & High-novelty	novelty = .90 effort = .88	.58 .48
	High-effort & Low-novelty	novelty = .92 effort = .79	.71 .32
	High-effort & Medium-novelty	novelty = .77 effort = .89	.33 .29
	High-effort & High-novelty	novelty = .91 effort = .94	.45 .45
Clothing	Low-effort & Low-novelty	novelty = .96 effort = .04	.71 .71
	Low-effort & Medium-novelty	novelty = .96 effort = .96	.73 .62
	Low-effort & High-novelty	novelty = .95 effort = .84	.66 .66
	Medium-effort & Low-novelty	novelty = .97 effort = .73	.78 .66
	Medium-effort & Medium-novelty	novelty = .98 effort = .82	.72 .72
	Medium-effort & High-novelty	novelty = .94 effort = .88	.82 .46
	High-effort & Low-novelty	novelty = .96 effort = .96	.61 .61
	High-effort & Medium-novelty	novelty = .95 effort = .78	.64 .56
	High-effort & High-novelty	novelty = .95 effort = .67	.45 .45
Health	Low-effort & Low-novelty	novelty = .97 effort = .91	.78 .72
	Low-effort & Medium-novelty	novelty = .95 effort = .90	.69 .69
	Low-effort &	novelty = .94	.46

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	High-novelty	effort = .94	.49

Topic	Situation	Interitem Reliability	Temporal Stability
Health	Medium-effort & Low-novelty	novelty = .91 effort = .88	.83 .83
	Medium-effort & Medium-novelty	novelty = .92 effort = .84	.66 .69
	Medium-effort & High-novelty	novelty = .94 effort = .93	.38 .38
	High-effort & Low-novelty	novelty = .93 effort = .91	.52 .63
	High-effort & Medium-novelty	novelty = .96 effort = .89	.78 .78
	High-effort & High-novelty	novelty = .89 effort = .95	.31 .32
Elderly	Low-effort & Low-novelty	novelty = .96 effort = .80	.79 .79
	Low-effort & Medium-novelty	novelty = .95 effort = .87	.84 .80
	Low-effort & High-novelty	novelty = .89 effort = .93	.69 .69
	Medium-effort & Low-novelty	novelty = .94 effort = .93	.77 .64
	Medium-effort & Medium-novelty	novelty = .96 effort = .86	.62 .62
	Medium-effort & High-novelty	novelty = .89 effort = .93	.25 .58
	High-effort & Low-novelty	novelty = .96 effort = .88	.73 .73
	High-effort & Medium-novelty	novelty = .92 effort = .94	.61 .74
	High-effort & High-novelty	novelty = .82 effort = .88	.46 .63

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